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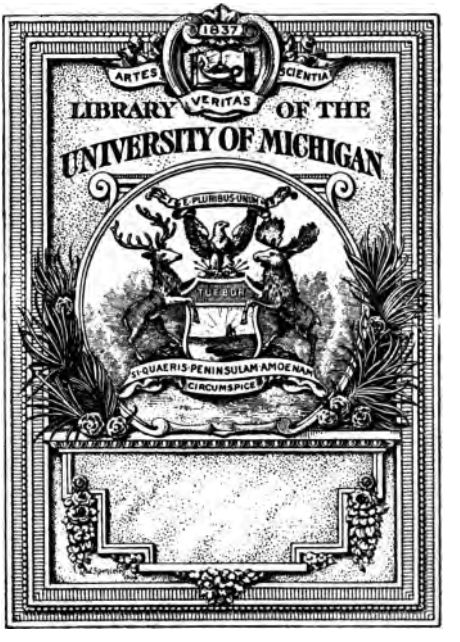
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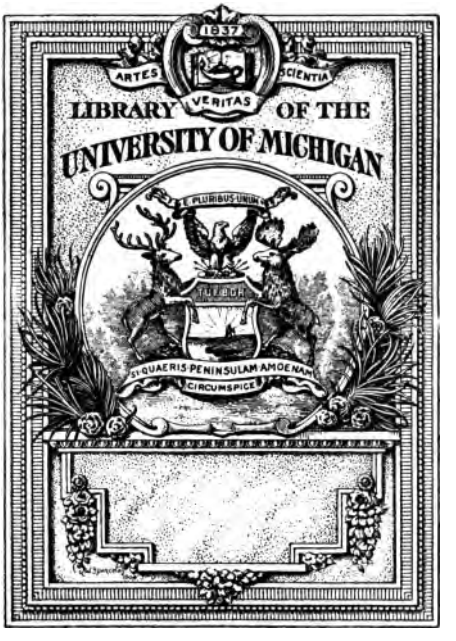
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THE UNIVERSITY OF CHICAGO
PRESENTED BY JOHN D. ROCKEFELLER

THE NATURE OF HYPOTHESIS

A DISSERTATION

SUBMITTED TO THE FACULTY OF THE GRADUATE SCHOOL OF ARTS AND LITERATURE IN CANDIDACY FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

(DEPARTMENT OF PHILOSOPHY)

BY
MYRON LUCIUS ARNETT

CHICAGO
1903

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1903

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VII

THE NATURE OF HYPOTHESIS

In the various discussions of the hypothesis which have appeared in works on inductive logic and in writings on scientific method, its structure and function have received considerable attention, while its origin has been comparatively neglected. The hypothesis has generally been treated as that part of scientific procedure which marks the stage where a definite plan or method is proposed for dealing with new or unexplained facts. It is regarded as an invention for the purpose of explaining the given, as a definite conjecture which is to be tested by an appeal to experience to see whether deductions made in accordance with it will be found true in fact. The function of the hypothesis is to unify, to furnish a method of dealing with things, and its structure must be suitable to this end. It must be so formed that it will be likely to prove valid, and writers have formulated various rules to be followed in the formation of hypotheses. These rules state the main requirements of a good hypothesis, and are intended to aid in a general way by pointing out certain limits within which it must fall.

In respect to the origin of the hypothesis, writers have usually contented themselves with pointing out the kind of situations in which hypotheses are likely to appear. But after this has been done, after favorable external conditions have been given, the rest must be left to "genius," for hypotheses arise as "happy guesses," for which no rule or law can be given. In fact, the genius differs from the ordinary plodding mortal in just this ability to form fruitful

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hypotheses in the midst of the same facts which to other less gifted individuals remain only so many disconnected experiences.

This unequal stress which has been laid on the structure and function of the hypothesis in comparison with its origin may be attributed to three reasons: (1) The facts, or data, which constitute the working material of hypotheses are regarded as given to all alike, and all alike are more or less interested in systematizing and unifying experience. The purpose of the hypothesis and the opportunity for forming it are thus practically the same for all, and hence certain definite rules can be laid down which will apply to all cases where hypotheses are to be employed. (2) But beyond this there seems to be no clue that can be formulated. There is apparently a more or less open acceptance of the final answer of the boy Zerah Colburn, who, when pressed to give an explanation of his method of instantaneous calculation, exclaimed in despair: "God put it into my head, and I can't put it into yours."[1] (3) And, furthermore, there is very often a strong tendency to disregard investigation into the origin of that which is taken as given, for, since it is already present, its origin, whatever it may have been, can have nothing to do with what it is now. The facts, the data, are *here*, and must be dealt with as they *are*. Their past, their history or development, is entirely irrelevant. So, even if we could trace the hypothesis farther back on the psychological side, the investigation would be useless, for the rules to which a good hypothesis must conform would remain the same.

Whether or not it can be shown that Zerah Colburn's ultimate explanation is needed in logic as little as Laplace asserted a similar one to be required in his celestial me-

[1] DE MORGAN, *Budget of Paradoxes*, pp. 55, 56; quoted by WELTON, *Logic*, Vol. II, p. 60.

chanics, it may at least be possible to defer it to some extent by means of a further psychological inquiry. It will be found that psychological inquiry into the origin of the hypothesis is not irrelevant in respect to an understanding of its structure and function; for origin and function cannot be understood apart from each other, and, since structure must be adapted to function, it cannot be independent of origin. In fact, origin, structure, and function are organically connected, and each loses its meaning when absolutely separated from each other. It will be found, moreover, that the data which are commonly taken as the given material are not something to which the hypothesis is subsequently applied, but that, instead of this external relation between data and hypothesis, the hypothesis exercises a directive function in determining what are the data. In a word, the main object of this discussion will be to contend against making a merely convenient and special way of regarding the hypothesis a full and adequate one. Though we speak of facts and of hypotheses that may be applied to them, it must not be forgotten that there are no facts which remain the same whatever hypothesis be applied to them; and that there are no hypotheses which are hypotheses at all except in reference to their function in dealing with our subject-matter in such a way as to facilitate its factual apprehension. Data are selected in order to be determined, and hypotheses are the ways in which this determination is carried on. If, as we shall attempt to show, the relation between data and hypothesis is not external, but strictly correlative, it is evident that this fact must be taken into account in questions concerning deduction and induction, analytic and synthetic judgments, and the criterion of truth. Its bearing must be recognized in the investigation of metaphysical problems as well, for reality cannot be independent of the knowing process. In a word, the purpose of this discussion of the

hypothesis is to determine its nature a little more precisely through an investigation of its rather obscure origin, and to call attention to certain features of its function which have not generally been accorded their due significance.

I

The hypothesis as predicate.—It is generally admitted that the function of the hypothesis is to provide a way of dealing with the data or subject-matter which we need to organize. In this use of the hypothesis it appears in the rôle of predicate in a judgment of which the data, or facts, to be construed constitute the subject.

In his attempts to reduce the movements of the planets about the sun to some general formula, Kepler finally hit upon the law since known as Kepler's law, viz., that the squares of the periodic times of the several planets are proportional to the cubes of their mean distances from the sun. This law was first tentatively advanced as a hypothesis. Kepler was not certain of its truth till it had proved its claim to acceptance. Neither did Newton have at first any great degree of assurance in regard to his law of gravitation, and was ready to give it up when he failed in his first attempt to test it by observation of the moon. And the same thing may be said about the caution of Darwin and other investigators in regard to accepting hypotheses. The only reason for their extreme care in not accepting at once their tentative formulations or suggestions was the fear that some other explanation might be the correct one. This rejection of other possibilities is the negative side of the matter. We become confident that our hypothesis is the right one as we lose confidence in other possible explanations; and it might be added, without falling into a circle, that we lose confidence in the other possibilities as we become more convinced of our hypothesis.

It appears that such may be the relation of the positive and negative sides in case of such elaborate hypotheses as those of Kepler and Newton; but is it true where our hypotheses are more simple? It is not easy to understand why the fact that the hypothesis is more simple, and the time required for its formulation and test a good deal shorter, should materially change the state of affairs. The question remains: Why, if there is no opposition, should there be any uncertainty? In all instances, then, the hypothesis appears as one among other possible predicates which may be applied to our data taken as subject-matter of a judgment.

The predicate as hypothesis.—Suppose, then, the hypothesis is a predicate; is the predicate necessarily a hypothesis? This is the next question we are called upon to answer, and, since the predicate cannot very well be taken aside from the judgment, our question involves the nature of the judgment.

While it will not be necessary to give a very complete account of the various definitions of the judgment that might be adduced, still the mention of a few of the more prominent ones may serve to indicate that something further is needed. In definitions of the judgment sometimes the subjective side is emphasized, sometimes the objective side, and in other instances there are attempts to combine the two. For instance, Lotze regards the judgment as the idea of a unity or relation between two concepts, with the further implication that this connection holds true of the object referred to. J. S. Mill says that every proposition either affirms or denies existence, coexistence, sequence, causation, or resemblance. Trendelenburg regards the judgment as a form of thought which corresponds to the real connection of things, while Ueberweg states the case a little differently, and says that the essence of judgment consists in recognizing the

objective validity of a subjective connection of ideas. Royce points to a process of imitation and holds that in the judgment we try to portray by means of the ideas that enter into it. Ideas are imitative in their nature. Sigwart's view of the judgment is that in it we say something about something. With him the judgment is a synthetic process, while Wundt considers its nature analytic and holds that, instead of uniting, or combining, concepts into a whole, it separates them out of a total idea or presentation. Instead of blending parts into a whole, it separates the whole into its constituent parts. Bradley and Bosanquet both hold that in the judgment an ideal content comes into relation with reality. Bradley says that in every judgment reality is qualified by an idea, which is symbolic. The ideal content is recognized as such, and is referred to a reality beyond the act. This is the essence of judgment. Bosanquet seems to perceive a closer relation between idea and reality, for although he says that judgment is the "intellectual function which defines reality by significant ideas," he also tells us that "the subject is both in and out of the judgment, as Reality is both in and out of my consciousness."

In all these definitions of judgment the predicate appears as ideal. An ideal content is predicated of something, whether we regard this something as an idea or as reality beyond, or as reality partly within and partly without the act of judging; and it is ideal whether we consider it as one of the three parts into which judgments are usually divided, or whether we say, with Bosanquet and Bradley, that subject, predicate, and copula all taken together form a single ideal content, which is somehow applied to reality. Moreover, we not only judge about reality, but it seems to be quite immaterial to reality whether we judge concerning it or not.

Many of our judgments prove false. Not only do we err

in our judgments, but we often hesitate in making them for fear of being wrong; we feel there are other possibilities, and our predication becomes tentative. Here we have something very like the hypothesis, for our ideal content shows itself to be a tentative attempt in the presence of alternatives to qualify and systematize reality. It appears, then, on the basis of the views of the judgment that have been mentioned, that not only do we find the hypothesis taking its place as the predicate of a judgment, but the predicate is itself essentially of the nature of a hypothesis.

In the views of the judgment so far brought out, reality, with which it is generally admitted that the judgment attempts to deal in some way, appears to lie outside the act of judging. Now, everyone would say that we make some advance in judging, and that we have a better grasp of things after than before. But how is this possible if reality lies without or beyond our act of judging? Is the reality we now have the same that we had to begin with? If so, then we have made no advance as far as the real itself is concerned. If merely our conception of it has changed, then it is not clear why we may not be even worse off than before. If reality does lie beyond our judgment, then how, in the nature of the case, can we ever know whether we have approached it or have gone still farther away? To make any claim of approximation implies that we do reach reality in some measure, at least, and, if so, it is difficult to understand how it lies beyond, and is independent of, the act of judging.

Further analysis of judgment.— It remains to be seen whether a further investigation of the judgment will still show the predicate to be a hypothesis. It is evident that in some cases the judgment appears at the end of a more or less pronounced reflective process, during which other possible judgments have suggested themselves, but have been

rejected. The history of scientific discovery is filled with cases which illustrate the nature of the process by which a new theory is developed. For instance, in Darwin's *Formation of Vegetable Mould through the Action of Earth Worms*, we find the record of successive steps in the development of his hypothesis. Darwin suspected from his observations that vegetable mold was due to some agency which was not yet determined. He reasoned that if vegetable mold is the result of the life-habits of earthworms, *i. e.*, if earth is brought up by them from beneath the surface and afterward spread out by wind and rain, then small objects lying on the surface of the ground would tend to disappear gradually below the surface. Facts seemed to support his theory, for layers of red sand, pieces of chalk, and stones were found to have disappeared below the surface in a greater or less degree. A common explanation had been that heavy objects tend to sink in soft soil through their own weight, but the earthworm hypothesis led to a more careful examination of the data. It was found that the weight of the object and the softness of the ground made no marked difference, for sand and light objects sank, and the ground was not always soft. In general, it was shown that where earthworms were found vegetable mold was also present, and *vice versa*.

In this investigation of Darwin's the conflicting explanations of sinking stones appear within the main question of the formation of vegetable mold by earthworms. The facts that disagreed with the old theory about sinking stones were approached through this new one. But the theories had something in common, viz., the disappearance of the stones or other objects: they differed in their further determination of this disappearance. In this case it may seem as if the facts which were opposed to the current theory of sinking stones were seen to be discrepant only after the earthworm hypothesis had been advanced; the conflict between the new facts

and the old theory appears to have arisen through the influence of the new theory.

There are cases, however, where the facts seem clearly to contradict the old theory and thus give rise to a new one. For example, we find in Darwin's introduction to his *Origin of Species* the following: "In considering the origin of species it is quite conceivable that a naturalist reflecting on the mental affinities of organic beings, on their embryological relations, their geographical distribution, geological succession, and other such facts, might come to the conclusion that species had not been independently created but had descended, like varieties, from other species." It would seem from this statement that certain data were found for which the older theory of independent creation did not offer an adequate explanation. And yet the naturalist would hardly "reflect" on all these topics in a comparative way unless some other mode of interpretation were already dawning upon him, which led him to review the accepted reflections or views.

As a more simple illustration, we may cite the common experience of a person who is uncertain concerning the identity of an approaching object, say, another person. At first he may not be sure it is a person at all. He then sees that it is someone, and as the person approaches he is inclined to believe him to be an acquaintance. As the supposed acquaintance continues to approach, the observer may distinguish certain features that cause him to doubt, and then relinquish his supposition that it is an acquaintance. Or, he may conclude at once that the approaching person is another individual he knows, and the transition may be so readily made from one to the other that it would be difficult to determine whether the discordant features are discordant before the new supposition arises, or whether they are not recognized as conflicting till this second person is in mind.

Or, again, the identification of the new individual and the discovery of the features that are in conflict with the first supposition may appear to go on together.

Now, marked lines of likeness appear between this relatively simple judgment and the far more involved ones of scientific research. In the more extended scientific process we find data contradicting an old theory and a new hypothesis arising to account for them. The hypothesis is tested, and along with its verification we have the rejection, or rather the modification, of the old theory. Similarly, in case of the approaching stranger all these features are present, though in less pronounced degree. In scientific investigation there is an interval of testing by means of more careful consideration of the data and even actual experimentation. Before an explanation is accepted subject to test, a number of others may have been suggested and rejected. They may not have received even explicit recognition. In case of the identification of the stranger this feature is also present. Between two fairly definite attempts to identify the mind does not remain a mere blank or stationary, but other possible identifications may be suggested which do not have sufficient plausibility to command serious attention; they are only comparatively brief suggestions or tendencies.

It is to be noted that in all these instances the first supposition was not *entirely* abandoned, but was modified and more exactly determined. (Why it could not be wholly false and the new one wholly new, will be considered later in connection with discussion of the persistence and reformation of habit.) There was such a modification of the old theory as would meet the requirements of the new data, and the new explanations thus contained both old and new features.

We have seen that the predicate of the scientific judg-

ment is a hypothesis which is consciously applied to certain data. If the similarity between the scientific judgment and the more immediate and simple judgment is to be maintained, it is clear that the predicate of the simple judgment must be of like nature. The structure of the two varieties of judgment differs only in the degree of explicitness which the hypothesis acquires. That is, the predicate of a judgment, as such, is ideal; it is meaning, significant quality. If conditions are such as to make the one judging hesitant or doubtful the mind wavers; the predicate is not applied at once to the determination or qualification of data, and hence comes to more distinct consciousness on its own account. From being "ideal," it becomes *an* idea. Yet its sole purpose and value remains in its possible use to interpret data. Let the idea remain detached, and let the query whether it be a true predicate (*i. e.*, really fit to be employed in determining the present data) become more critical, and the idea becomes clearly a hypothesis.[1] In other words, the hypothesis is just the predicate-function of judgment definitely apprehended and regarded with reference to its nature and adequacy.

Psychological analysis of judgment.—This hypothetical nature of the predicate will be even more apparent after a further psychological analysis, which, while applying more directly to the simpler and more immediate judgments, may be extended to the more involved ones as well.

In psychological terms, we may say, in explanation of the judging process, that some stimulus to action has failed to function properly as a stimulus, and that the activity

[1]Advanced grammarians treat this matter in a way which should be instructive to logicians. The hypothesis, says SWEET (§ 295 of *A New English Grammar, Logical and Historical*, Oxford, 1892), suggests an affirmation or negation "as objects of thought." "In fact, we often say *supposing* (that is, 'thinking') *it is true*, instead of *if it is true*." In a word, the hypothetical judgment as such puts explicitly before us the content of thought, of the predicate or hypothesis; and in so far is a moment in judgment rather than adequate judgment itself.

which was going on has thus been interrupted. Response in the accustomed way has failed. In such a case there arises a division in experience into sensation content as subject and ideal content as predicate. In other words, an activity has been going on in accordance with established habits, but upon failure of the accustomed stimulus to be longer an adequate stimulus this particular activity ceases, and is resumed in an integral form only when a new habit is set up to which the new or altered stimulus is adequate. It is in this process of reconstruction that subject and predicate appear. Sensory quality marks the point of stress, or seeming arrest, while the ideal or imaged aspect defines the continuing activity as projected, and hence that with which start is to be made in coping with the obstacle. It serves as standpoint of regard and mode of indicated behavior. The sensation stands for the interrupted habit, while the image stands for the new habit, that is, the new way of dealing with the subject-matter.[1]

It appears, then, that the purpose of the judgment is to obtain an adequate stimulus in that, when stimulus and response are adjusted to each other, activity will be resumed. But if this reconstruction and response were to follow at once, would there be any clearly defined act of judging at all? In such a case there would be no judgment, properly speaking, and no occasion for it. There would be simply a ready transition from one line of activity to another; we should have changed our method of reaction easily and readily to meet the new requirements. On the one hand, our subject-matter would not have become a clearly recognized datum with which we must deal; on the other hand, there would be no ideal method of construing it.[2] Activity

[1] This carries with it, of course, the notion that "sensation" and "image" are not distinct psychical existences in themselves, but are distinguished logical forces.

[2] Concerning the strict correlativity of subject and predicate, data and hypothesis, see pp. 182,183.

would have changed without interruption, and neither subject nor predicate would have arisen.

In order that judgment may take place there must be interruption and suspense. Under what conditions, then, is this suspense and uncertainty possible? Our reply must be that we hesitate because of more or less sharply defined alternatives; we are not sure which predicate, which method of reaction, is the right one. The clearness with which these alternatives come to mind depends upon the degree of explicitness of the judgment, or, more exactly, the explicitness of the judgment depends upon the sharpness of these alternatives. Alternatives may be carefully weighed one against the other, as in deliberative judgments; or they may be scarcely recognized as alternatives, as in the case in the greater portion of our more simple judgments of daily conduct.

The predicate is essentially hypothetical.—If we review in a brief résumé the types of judgment we have considered, we find in the explicit scientific judgment a fairly well-defined subject-matter which we seek further to determine. Different suggestions present themselves with varying degrees of plausibility. Some are passed by as soon as they arise. Others gain a temporary recognition. Some are explicitly tested with resulting acceptance or rejection. The acceptance of any one explanation involves the rejection of some other explanation. During the process of verification or test the newly advanced supposition is recognized to be more or less doubtful. Besides the hypothesis which is tentatively applied there is recognized the possibility of others. In the disjunctive judgment these possible reactions are thought to be limited to certain clearly defined alternatives, while in the less explicit judgments they are not so clearly brought out. Throughout the various forms of judgment, from the most complex and deliberate down to

the most simple and immediate, we found that a process could be traced which was like in kind and varied only in degree. And, finally, in the most immediate judgments where some of these features seem to disappear, the same account not only appears to be the most reasonable one, but there is the additional consideration, from the psychological side, that were not the judgment of this doubtful, tentative character, it would be difficult to understand how there could be judgment as distinct from a reflex. It appears, then, that throughout, the predicate is essentially of the nature of a hypothesis for dealing with the subject-matter. And, however simple and immediate, or however involved and prolonged, the judgment may be, it is to be regarded as essentially a process of reconstruction which aims at the resumption of an interrupted experience; and when experience has become itself a consciously intellectual affair, at the restoration of a unified objective situation.

II

Criticism of certain views concerning the hypothesis.— The explanation we have given of the hypothesis will enable us to criticise the treatment it has received from the empirical and the rationalistic schools. We shall endeavor to point out that these schools have, in spite of their opposed views, an assumption in common—something given in a fixed, or non-instrumental way; and that consequently the hypothesis is either impossible or else futile.

Bacon is commonly recognized as a leader in the reactionary inductive movement, which arose with the decline of scholasticism, and will serve as a good example of the extreme empirical position. In place of authority and the deductive method, Bacon advocated a return to nature and induction from data given through observation. The new method which he advanced has both a positive and a

negative side. Before any positive steps can be taken, the mind must be cleared of the various false opinions and prejudices that have been acquired. This preliminary task of freeing the mind from "phantoms," or "eidola," which Bacon likened to the cleansing of the threshing-floor, having been accomplished, nature should be carefully interrogated. There must be no hasty generalization, for the true method "collects axioms from sense and particulars, ascending continuously and by degrees, so that in the end it arrives at the most general axioms." These axioms of Bacon's are generalizations based on observation, and are to be applied deductively, but the distinguishing feature of Bacon's induction is its carefully graduated steps. Others, too, had proceeded with caution (for instance Galileo), but Bacon laid more stress than they on the subordination of steps.

It is evident that Bacon left very little room for hypotheses, and this is in keeping with his aversion to anticipation of nature by means of "phantoms" of any sort; he even said explicitly that "our method of discovery in science is of such a nature that there is not much left to acuteness and strength of genius, but all degrees of genius and intellect are brought nearly to the same level."[1] Bacon gave no explanation of the function of the hypothesis; in his opinion it had no lawful place in scientific procedure and must be banished as a disturbing element. Instead of the reciprocal relation between hypothesis and data, in which hypothesis is not only tested in experience, but at the same time controls in a measure the very experience which tests it, Bacon would have a gradual extraction of general laws from nature through direct observation. He is so afraid of the distorting influence of conception that he will have nothing to do with conception upon any terms. So fearful is he of the influence of pre-judgment, of prejudice, that he will have

[1] *Novum Organum*, Vol. I, p. 61.

no judging which depends upon ideas, since the idea involves anticipation of the fact. Particulars are somehow to arrange and classify themselves, and to record or register, in a mind free from conception, certain generalizations. Ideas are to be registered derivatives of the given particulars. This view is the essence of empiricism as a logical theory. If the views regarding the logic of thought before set forth are correct, it goes without saying that such empiricism is condemned to self-contradiction. It endeavors to construct judgment in terms of its subject alone; and the subject, as we have seen, is always a co-respondent to a predicate — an idea or mental attitude or tendency of intellectual determination. Thus the subject of judgment can be determined only with reference to a corresponding determination of the predicate. Subject and predicate, fact and idea, are contemporaneous, not serial in their relations (see pp. 110–12).

Less technically the failure of Bacon's denial of the worth of hypothesis — which is in such exact accord with empiricism in logic — shows itself in his attitude toward experimentation and toward observation. Bacon's neglect of experimentation is not an accidental oversight, but is bound up with his view regarding the worthlessness of conception or anticipation. To experiment means to set out from an idea as well as from facts, and to try to construe, or even to discover, facts in accordance with the idea. Experimentation not only anticipates, but strives to make good an anticipation. Of course, this struggle is checked at every point by success or failure, and thus the hypothesis is continuously undergoing in varying ratios both confirmation and transformation. But this is not to make the hypothesis secondary to the fact. It is simply to remain true to the proposition that the distinction and the relationship of the two is a thoroughly contemporaneous one. But it is impossible to draw any fixed line between experimentation and scientific observa-

tions. To insist upon the need of systematic observation and collection of particulars is to set up a principle which is as distinct from the casual accumulation of impressions as it is from nebulous speculation. If there is to be observation of a directed sort, it must be with reference to some problem, some doubt, and this, as we have seen, is a stimulus which throws the mind into a certain attitude of response. Controlled observation is inquiry, it is search; consequently it must be search for something. Nature cannot answer interrogations excepting as such interrogations are put; and the putting of a question involves anticipation. The observer does not inquire about anything or look for anything excepting as he is after something. This search implies at once the incompleteness of the particular given facts, and the possibility—that is ideal—of their completion.

It was not long until the development of natural science compelled a better understanding of its actual procedure than Bacon possessed. Empiricism changed to experimentalism. With experimentalism inevitably came the recognition of hypotheses in observing, collecting, and comparing facts. It is clear, for instance,that Newton's fruitful investigations are not conducted in accordance with the Baconian notion. It is quite clear that his celebrated four rules for philosophizing[1] are in truth statements of certain principles which are to be observed in forming hypotheses. They imply that scientific technique had advanced to a point

[1]Newton's "Rules for Philosophizing" (*Principia*, Book III) are as follows:

Rule I. "No more causes of natural things are to be admitted than such as are both true, and sufficient to explain the phenomena of those things."

Rule II. "Natural effects of the same kind are to be referred as far as possible to the same causes."

Rule III. "Those qualities of bodies that can neither be increased nor diminished in intensity, and which are found to belong to all bodies within reach of our experiments are to be regarded as qualities of all bodies whatever."

Rule IV. "In experimental philosophy propositions collected by induction from phenomena are to be regarded either as accurately true or very nearly true notwithstanding any contrary hypothesis, till other phenomena occur, by which they are made more accurate, or are rendered subject to exceptions."

where hypotheses were such regular and indispensable factors that certain uniform conditions might be laid down for their use. The fourth rule in particular is a statement of the relative validity of hypothesis as such until there is ground for entertaining a contrary hypothesis.

The subsequent history of logical theory in England is conditioned upon its attempt to combine into one system the theories of empiristic logic with recognition of the procedure of experimental science. This attempt finds its culmination in the logic of John Stuart Mill. Of his interest in and fidelity to the actual procedure of experimental science, as he saw it, there can be no doubt. Of his good faith in concluding his *Introduction* with the words following there can be no doubt: "I can conscientiously affirm that no one proposition laid down in this work has been adopted for the sake of establishing, or with any reference for its fitness in being employed in establishing, preconceived opinions in any department of knowledge or of inquiry on which the speculative world is still undecided." Yet Mill was equally attached to the belief that ultimate reality, as it is for the human mind, is given in sensations, independent of ideas; and that all valid ideas are combinations and convenient ways of using such given material. Mill's very sincerity made it impossible that this belief should not determine, at every point, his treatment of the thinking process and of its various instrumentalities.

In Book III, chap. 14, Mill discusses the logic of explanation, and in discussing this topic naturally finds it necessary to consider the matter of the proper use of scientific hypotheses. This is conducted from the standpoint of their use as that is reflected in the technique of scientific discovery. In Book IV, chap. 2, he discusses "Abstraction or the Formation of Conceptions" — a topic which obviously involves the forming of hypotheses. In this chapter, his con-

sideration is conducted in terms, not of scientific procedure, but of general philosophical theory, and this point of view is emphasized by the fact that he is opposing a certain view of Dr. Whewell.

The contradiction between the statements in the two chapters will serve to bring out the two points already made, viz., the correspondent character of datum and hypothesis, and the origin of the latter in a problematic situation and its consequent use as an instrument of unification and solution. Mill first points out that hypotheses are invented to enable the deductive method to be applied earlier to phenomena; that it does this by suppressing the first of the three steps, induction, ratiocination, and verification. He states that:

> The process of tracing regularity in any complicated, and at first sight confused, set of appearances is necessarily tentative; we begin by making any supposition, even a false one, to see what consequences will follow from it; and by observing how these differ from the real phenomena, we learn what corrections to make in our assumption. . . . . *Neither induction nor deduction would enable us to understand even the simplest phenomena*, if we did not often commence by anticipating the results; by making a provisional supposition, at first essentially conjectural, as to some of the very notions which constitute the final object of the inquiry.[1]

If in addition we recognize that, according to Mill, our direct experience of nature always presents us with a complicated and confused set of appearances, we shall be in no doubt as to the importance of ideas as anticipations of a possible experience not yet had. Thus he says:

> The order of nature, as perceived at a first glance, presents at every instant a chaos followed by another chaos. We must decompose each chaos into single facts. We must learn to see in the chaotic antecedent a multitude of distinct antecedents, in the chaotic consequent a multitude of distinct consequents.[2]

[1] Book III, chap. 2, sec. 5; italics mine. The latter part of the passage, beginning with the words "If we did not often commence," etc., is quoted by Mill from Comte. The words "neither induction nor deduction would enable us to understand even the simplest phenomena" are his own.

[2] Book III, chap. 7, sec. 1.

In the next section of the same chapter he goes on to state that, having discriminated the various antecedents and consequents, we then "are to inquire which is connected with which." This requires a still further resolution of the complex and of the confused. To effect this we must vary the circumstances; we must modify the experience as given with reference to accomplishing our purpose. To accomplish this purpose we have recourse either to observation or to experiment: "We may either *find* an instance in nature *suited to our purposes*, or, by an artificial arrangement of circumstances, *make* one" (the italics in "suited to our purpose" are mine; the others are Mill's). He then goes on to say that there is no real logical distinction between observation and experimentation. The four methods of experimental inquiry are expressly discussed by Mill in terms of their worth in singling out and connecting the antecedents and consequents which actually belong together, from the chaos and confusion of direct experience.

We have only to take these statements in their logical connection with each other (and this connection runs through the entire treatment by Mill of scientific inquiry), to recognize the absolute necessity of hypothesis to undertaking any directed inquiry or scientific operation. Consequently we are not surprised at finding him saying that "the function of hypotheses is one which must be reckoned absolutely indispensable in science;" and again that "the hypothesis by suggesting observations and experiments puts us on the road to independent evidence."[1]

Since Mill's virtual retraction, from the theoretical point of view, of what is here said from the standpoint of scientific procedure, regarding the necessity of ideas is an accompaniment of his criticism of Whewell, it will put the discussion in better perspective if we turn first to Whewell's views.[2]

[1] Book III, chap. 14, secs. 4 and 5.

[2] WILLIAM WHEWELL, *The Philosophy of the Inductive Sciences*, London, 1840.

The latter began by stating a distinction which easily might have been developed into a theory of the relation of fact and idea which is in line with that advanced in this chapter, and indeed in this volume as a whole. He questions (chap. 2) the fixity of the distinction between theory and practice. He points out that what we term facts are in effect simply accepted inferences; and that what we call theories are describable as facts, in proportion as they become thoroughly established. A true theory is a fact. "All the great theories which have successively been established in the world are now thought of as facts." "The most recondite theories when firmly established are accepted as facts; the simplest facts seem to involve something of the nature of theory."

The conclusion is that the distinction is a historic one, depending upon the state of knowledge at the time, and upon the attitude of the individual. What is theory for one epoch, or for one inquirer in a given epoch, is fact for some other epoch, or even for some other more advanced inquirer in the same epoch. It is theory when the element of inference involved in judging any fact is consciously brought out; it is fact when the conditions are such that we have never been led to question the inference involved, or else, having questioned it, have so thoroughly examined into the inferential process that there is no need of holding it further before the mind, and it relapses into unconsciousness again. "If this greater or less consciousness of our own internal act be all that distinguishes fact from theory, we must allow that the distinction is still untenable" (untenable, that is to say, as a fixed separation). Again, "fact and theory have no essential difference except in the degree of their *certainty and familiarity*. Theory, when it becomes firmly established and steadily lodged in the mind becomes fact." (P. 45; italics mine.) And, of course, it is equally true that as fast as facts are suspected or doubted, certain aspects of them

are transferred into the class of theories and even of mere opinions.

I say this conception might have been developed in a way entirely congruous with the position of this chapter. This would have happened if the final distinction between fact and idea had been formulated upon the basis simply of the points, "relative certainty and familiarity." From this point of view the distinction between fact and idea is one purely relative to the doubt-inquiry function. It has to do with the evolution of an experience as regards its conscious surety. It has its origin in problematic situations. Whatever appears to us as a problem appears as contrasted with a possible solution. Whatever objects of thought refer particularly to the problematic side are theories, ideas, hypotheses; whatever relates to the solution side is surety, unquestioned familiarity, fact. This point of view makes the distinctions entirely relative to the exigencies of the process of reflective transformation of experience.

Whewell, however, had no sooner started in this train of thought than he turns his back upon it. In chap. 3 he transforms what he had proclaimed to be a relative, historic, and working distinction into a fixed and absolute one. He distinguishes between sensations and ideas, not upon a genetic basis with reference to establishing the conditions of further operation; but with reference to a fundamentally fixed line of demarkation between what is passively *given* to the mind and the *activity* put forth by the mind. Thus he reinstates in its most generalized and fixed, and therefore most vicious, form the separation which he has just rejected. Sensations are a brute unchangeable element of fact which exists and persists independent of ideas; an idea is a mode of mental operation which occurs and recurs in an independent individuality of its own. If he had carried out the line of thought with which he began, sensation as fact

would have been that residuum of familiarity and certainty which cannot be eliminated, however much else of an experience is dissolved in the inner conflict. Idea as hypothesis or theory would have been the corresponding element in experience which is necessary to redintegrate this residuum into a coherent and significant experience.

But since Whewell did not follow out his own line of thought, choosing rather to fall back on the Kantian antithesis of sense and thought, he had no sooner separated his fact and idea, his given datum and his mental relation, than he is compelled to get them together again. The idea becomes "a general relation which is imposed upon perception by an act of the mind, and which is different from anything which our senses directly offer to us" (p. 26). Such conceptions are necessary to connect the facts which we learn from our senses into truths. "The ideal conception which the mind itself supplies is superinduced upon the facts as they are originally presented to observation. Before the inductive truth is detected, the facts are there, but they are many and unconnected. The conception which the discoverer applies to them gives them connection and unity." (P. 42.) All induction, according to Whewell, thus depends upon superinduction — imposition upon sensory data of certain ideas or general relations existing independently in the mind.[1]

We do not need to present again the objections already offered to this view: the impossibility of any orderly stimulation of ideas by facts, and the impossibility of any check in the imposition of idea upon fact. "Facts" and conception are so thoroughly separate and independent that any sensory datum is indifferently and equally related to any conceivable idea. There is no basis for "superinducing"

[1] The essential similarity between Whewell's view and that of Lotze, already discussed (see chap. 3) is of course explainable on the basis of their common relationship to Kant.

one idea or hypothesis, rather than any other, upon any particular set of data.

In the chapter already referred to upon abstraction, or the formation of conceptions, Mill seizes upon this difficulty. Yet he and Whewell have one point in common: they both agree in the existence of a certain subject-matter which is given for logical purposes quite outside of the logical process itself. Mill agrees with Whewell in postulating a raw material of pure sensational data. In criticising Whewell's theory of superinduction of idea upon fact, he is therefore led to the opposite assertion of the complete dependence of ideas as such upon the given facts as such — in other words, he is led to a reiteration of the fundamental Baconian empiricism; and thus to a virtual retraction of what he had asserted regarding the necessity of ideas to fruitful scientific inquiry, whether in the way of observation or experimentation. The following quotation gives a fair notion of the extent of Mill's retraction:

> The conceptions then which we employ for the colligation and methodization of facts, do not develop themselves from within, *but are impressed upon the mind from without;* they are never obtained otherwise than by way of comparison and abstraction, and, in the most important and most numerous cases, are evolved by abstraction *from the very phenomena which it is their office to colligate.*[1]

Even here Mill's sense for the positive side of scientific inquiry suffices to reveal to him that the "facts" are somehow inadequate and defective, and are in need of assistance from ideas — and yet the ideas which are to help out the facts are to be the impress of the unsure facts! The contradiction comes out very clearly when Mill says: "The really difficult cases are those in which the conception destined to create light and order out of darkness and confu-

[1] *Logic*, Book IV, chap. 2, sec. 2; italics mine.

sion has to be sought for among the very phenomena which it afterward serves to arrange."[1]

Of course, there is a sense in which Mill's view is very much nearer the truth than is Whewell's. Mill at least sees that "idea" must be relevant to the facts or data which it is to arrange, which are to have "light and order" introduced into them by means of the idea. He sees clearly enough that this is impossible save as the idea develops *within* the same experience in which the "dark and confused" facts are presented. He goes on to show correctly enough how conflicting data lead the mind to a "confused feeling of an analogy" between the data of the confused experience and of some other experience which is orderly (or already colligated and methodized); and how this vague feeling, through processes of further exploration and comparison of experiences, gets a clearer and more adequate form until we finally accept it. He shows how in this process we continually judge of the worth of the idea which is in process of formation, by reference to its appropriateness to *our purpose.* He goes so far as to say: "The question of appropriateness is relative to the *particular object we have in view.*"[2] He sums up his discussion by stating: "We cannot frame good general conceptions beforehand. That the conception we have obtained is the one we want can only be known when we have *done the work for the sake of which we wanted it.*"[3]

This all describes the actual state of the case, but it is consistent only with a logical theory which makes the distinction between fact and hypothesis instrumental in the transformation of experience from a confused into an organized form; not with Mill's notion that sensations are somehow finally and completely given as ultimate facts, and

[1] *Ibid.*

[2] *Ibid.*, sec. 4; in sec. 6 he states even more expressly that any conception is appropriate in the degree in which it "helps us toward what we wish to understand."

[3] *Ibid.*, sec. 6; italics mine.

that ideas are mere re-registrations of such facts. It is perfectly just to say that the hypothesis is impressed upon the mind (in the sense that any notion which occurs to the mind is impressed) *in the course* of an experience. It is well enough, if one define what he means, to say that the hypothesis is impressed (that is to say, occurs or is suggested) through the medium of given facts, or even of sensations. But it is equally true that the *facts* are presented and that *sensations* occur within the course of an experience which is larger than the bare facts, because involving the conflicts among them and the corresponding intention to treat them in some fashion which will secure a unified experience. Facts get power to suggest ideas to the mind — to "impress" — only through their position in an entire experience which is in process of disintegration and of reconstruction — their "fringe" or feeling of tendency is quite as factual as they are. The fact that "the conception we have obtained is the one we want can be known only when we have done the work for the sake of which we wanted it," is enough to show that it is not bare facts, but facts in relation to want and purpose and purpose in relation to facts, which originate the hypothesis.

It would be interesting to follow the history of discussion of the hypothesis since the time of Whewell and of Mill, particularly in the writings of Jevons, Venn, and Bosanquet. This history would refine the terms of our discussion by introducing more complex distinctions and relations. But it would be found, I think, only to refine, not to introduce any fundamentally new principles. In each case, we find the writer struggling with the necessity of distinguishing between fact and idea; of giving the fact a certain primacy with respect to testing of idea and of giving the idea a primacy with respect to the significance and orderliness of the fact; and of holding throughout to a relationship of idea with

fact so intimate that the idea develops only by being "compared" with facts (that is, used in construing them), and facts get to be known only as they are "connected" through the idea—and we find that what is a maze of paradoxes and inconsistencies from an absolute, from a non-historic standpoint, is a matter of course the moment it is looked at from the standpoint of experience engaged in self-transformation of meaning through conflict and reconstitution.

But we can only note one or two points. Jevons's "infinite ballot-box" of nature which is absolutely neutral as to any particular conception or idea, and which accordingly requires as its correlate the formation of every possible hypothesis (all standing in themselves upon the same level of probability) is an interesting example of the logical consequences of feeling the need of both fact and hypothesis for scientific procedure and yet regarding them as somehow arising independently of each other. It is an attempt to combine extreme empiricism and extreme rationalism. The process of forming hypotheses and of deducing their rational consequences goes on at random, because the disconnectedness of facts as given is so ultimate that the facts suggest one hypothesis no more readily than another. Mathematics, in its two forms of measurements as applied to the facts, and of calculation as applied in deduction, furnishes Jevons the bridge by which he finally covers the gulf which he has first himself created. Venn's theory requires little or no restatement to bring it into line with the position taken in the text. He holds to the origin of hypothesis in the original practical needs of mankind, and to its gradual development into present scientific form.[1] He states expressly:

The *distinction between what is known and what is not known is essential to Logic*, and peculiarly characteristic of it in a degree not to be found in any other science. Inference is the

[1] VENN, *Empirical Logic*, p. 383.

process of passing from one to the other; from facts which we had accepted as premises, to those which we have not yet accepted, *but are in the act of doing so by the very process in question.* No scrutiny of the facts themselves, regarded as objective, can ever detect these characteristics of their greater or less familiarity to our minds. We must introduce also the subjective element if we wish to give any adequate explanation of them.[1]

Venn, however, does not attempt a thoroughgoing statement of logical distinctions, relations, and operations, as parts "of the act of passing from the unknown to the known." He recognizes the relation of reflection to a historic process, which we have here termed "reconstruction," and the origin and worth of hypothesis as a tool in the movement, but does not carry his analysis to a systematic form.

III

Origin of the hypothesis.— In our analysis of the process of judgment, we attempted to show that the predicate arises in case of failure of some line of activity going on in terms of an established habit. When the old habit is checked through failure to deal with new conditions (*i. e.*, when the situation is such as to stimulate two habits with distinct aims) the problem is to find a new method of response— that is, to co-ordinate the conflicting tendencies by building up a single aim which will function the existing situation. As we saw that, in case of judgment, habit when checked became ideal, an idea, so the new habit is first formalized as an ideal type of reaction and is the hypothesis by which we attempt to construe new data. In our inquiry as to how this formulation is effected, *i. e.*, how the hypothesis is developed, it will be convenient to take some of the currently accepted statements as to their origin, and show how these statements stand in reference to the analysis proposed.

[1] VENN, *Empirical Logic*, p. 25; italics mine.

Enumerative induction and allied processes.—It is pointed out by Welton[1] that the various ways in which hypotheses are suggested may be reduced to three classes, viz., enumerative induction, conversion of propositions, and analogy. Under the head of "enumeration" he reminds us that "every observed regularity of connection between phenomena suggests a question as to whether it is universal." There are numerous instances of this in mathematics. For example, it is noticed that $1+3=2^2$, $1+3+5=3^2$, $1+3+5+7=4^2$, etc.; and one is led to ask whether there is any general principle involved, so that the sum of the first n odd numbers will be n^2, where n is any number, however great. In this early form of inductive inference there are two divergent tendencies. One is the tendency to complete enumeration. This *tendency* is clearly ideal—it transcends the facts as given. To look for all the cases is thus itself an experimental inquiry, based upon a hypothesis which it endeavors to test. But in most cases enumeration can be only incomplete, and we are able to reach nothing better than probability. Hence the other tendency in the direction of an analysis of content in search for a principle of connection in the elements in any *one* case. For if a characteristic belonging to a number of individuals suggests a class where it belongs to all individuals, it must be that it is found in every individual as such. The hypothesis of complete class involves a hypothesis as to the character of each individual in the class. Thus a hypothesis as to extension transforms itself into one as to intension.

But it is analogy which Welton considers "the chief source from which new hypotheses are drawn." In the second tendency mentioned under enumerative induction, that is, the tendency to analysis of content or intension, we are naturally led to analogy, for in our search for the char-

[1] WELTON, *Manual of Logic*, Vol. II, chap. 3.

acteristic feature which determines classification among the concrete particulars our first step will be an inference by analogy. In analogy attention is turned from the number of observed instances to their character, and, because particulars have some feature in common, they are supposed to be the same in still other respects. While the best we can reach in analogy is probability, the arguments may be such as to result in a high degree of certainty. The form of the argument is valuable in so far as we are able to distinguish between essential and nonessential characteristics on which to base our analogy. What is essential and what nonessential depends upon the particular end we have in view.

In addition to enumerative induction, which Welton has mentioned, it is to be noted that there are a number of other processes which are very similar to it in that a number of particulars appear to furnish a basis for a general principle or method. Such instances are common in induction, in instruction, and in methods of proof.

If one is to be instructed in some new kind of labor, he is supposed to acquire a grasp of the method after having been shown in a few instances how this particular work is to be done; and, if he performs the manipulations himself, so much the better. It is not asked why the experience of a few cases should be of any assistance, for it seems self-evident that an experienced man, a man who has acquired the skill, or knack, of doing things, should deal better with all other cases of similar nature.

There is something very similar in inductive proofs, as they are called. The inductive proof is common in algebra. Suppose we are concerned in proving the law of expansion of the binomial theorem. We show by actual calculation that, if the law holds good for the nth power, it is true for the $n+$ first power. That is, if it holds for any power, it holds for the next also. But we can easily show that it does

hold for, say, the second power. Then it must be true for the third, and hence for the fourth, and so on. Whether this law, though discovered by inductive processes, depends on deduction for the conclusiveness of its proof, as Jevons holds;[1] whether, as Erdmann[2] contends, the proof is thoroughly deductive; or whether Wundt[3] is right in maintaining that it is based on an exact analogy, while the fundamental axioms of mathematics are inductive, it is clear that in such proofs a few instances are employed to give the learner a start in the right direction. Something suggests itself, and is found true in this case, in the next, and again in the next, and so on. It may be questioned whether there is usually a very clear notion of what is involved in the "so on." To many it appears to mark the point where, after having been taken a few steps, the learner is carried on by the acquired momentum somewhat after the fashion of one of Newton's laws of motion. Whether the few successive steps are an integral part of the proof or merely serve as illustration, they are very generally resorted to. In fact, they are often employed where there is no attempt to introduce a general term such as n, or k, or l, but the few individual instances are deemed quite sufficient. Such, for instance, is the custom in arithmetical processes. We call attention to these facts in order to show that successive cases are utilized in the course of explanation as an aid in establishing the generality of a law.

In geometry we find a class of proofs in which the successive steps seem to have great significance. A common proof of the area of the circle will serve as a fair example. A regular polygon is circumscribed about the circle. Then as the number of its sides are increased its area will approach

[1] W. S. JEVONS, *Principles of Science*, pp. 231, 232.

[2] B. ERDMANN, "Zur Theorie des Syllogismus und der Induktion," *Philosophische Abhandlungen*, Vol. VI, p. 230.

[3] WUNDT, *Logik*, 2d ed., Vol. II, p. 131.

that of the circle, as its perimeter approaches the circumference of the circle. The area of the circle is thus inferred to be πR^2, since the area of the polygon is always $\frac{1}{2}R \times$ perimeter, and in case of the circle the circumference $= 2\pi R$. Here again we get under such headway by means of the polygon that we arrive at the circle with but little difficulty. Had we attempted the transition at once, say, from a circumscribed square, we should doubtless have experienced some uncertainty and might have recoiled from what would seem a rash attempt; but as the number of the sides of our polygon approach infinity—that mysterious realm where many paradoxical things become possible—the transition becomes so easy that our polygon is often said to have truly become a circle.

Similarly, some statements of the infinitesimal calculus rest on the assumption that slight degrees of difference may be neglected. Though the more modern theory of limits has largely displaced this attitude in calculus and has also changed the method of proof in such geometrical problems as the area of the circle, the underlying motive seems to have been to make transitions easy, and thus to make possible a continued application of some particular method or way of dealing with things.

But granted that this is all true, what has it to do with the origin of the hypothesis? It seems likely that the hypothesis may be suggested by a few successive instances; but are these to be classed with the successive steps in proof to which we have referred? In the first place, we attempt to prove our hypothesis because we are not sure it is true; we are not satisfied that there are no other tenable hypotheses. But if we do test it, is not such test enough? It depends upon how thorough a grasp we have of the situation; but, in general, each test case adds to its probability. The value of tests lies in the fact that they strengthen and

tend to confirm our hypothesis by checking the force of alternatives. One instance is not sufficient because there are other possible incipient hypotheses, or more properly tendencies, and the enumeration serves to bring one of these tendencies into prominence in that it diminishes other vague and perhaps subconscious tendencies and strengthens the one which suddenly appears as the mysterious product of genius.

The question might arise why the mere repetition of conflicting tendencies would lead to a predominance of one of them. Why would they not all remain in conflict and continue to check any positive result? It is probably because there never is any absolute equilibrium. The successive instances tend to intensify and bring into prominence some tendency which is already taking a lead, so to speak. And it may be said further in this connection that only as seen from the outside, only as a mechanical view is taken, does there appear to be an excluding of definitely made out alternatives.

In explanation of the part played by analogy in the origin of hypotheses, Welton points out that a mere number of instances do not take us very far, and that there must be some "*specification* of the instances as well as numbering of them," and goes on to show that the argument by enumerative induction passes readily into one from analogy, as soon as attention is turned from the number of the observed instances to their character. It is not necessary, however, to pass to analogy through enumerative induction. "When the instances presented to observation offer immediately the characteristic marks on which we base the inference to the connection of S and P, we can proceed at once to an inference from analogy, without any preliminary enumeration of the instances."[1]

[1] WELTON, *Manual of Logic*, Vol. II, p. 72.

Welton, and logicians generally, regard analogy as an inference on the basis of partial identity. Because of certain common features we are led to infer a still greater likeness.

Both enumerative induction and analogy are explicable in terms of habit. We saw in our examination of enumerative induction that a form of reaction gains strength through a series of successful applications. Analogy marks the presence of an identical element together with the tendency to extend this "partial identity" (as it is commonly called) still farther. In other words, in analogy it is suggested that a type of reaction which is the same in certain respects may be made similar in a greater degree. In enumerative induction we lay stress on the number of instances in which the habit is applied. In analogy we emphasize the content side and take note of the partial identity. In fact, the relation between enumerative induction and analogy is of the same sort as that existing between association by contiguity and association by similarity. In association by contiguity we think of the things associated as merely standing in certain temporal or spatial relations, and disregard the fact that they were elements in a larger experience. In case of association by similarity we regard the like feature in the things associated as a basis for further correction.

In conversion of propositions we try to reverse the direction of the reaction, so to speak, and thereby to free the habit, to get a mode of response so generalized as to act with a minimum cue. For instance, we can deal with A in a way called B, or, in other words, in the same way that we did with other things called B. If we say, "Man is an animal," then to a certain extent the term "animal" signifies the way in which we regard "man." But the question arises whether we can regard all animals as we do man. Evidently not, for the reaction which is fitting in

case of animals would be only partially applicable to man. With the animals that are also men we have the beginning of a habit which, if unchecked, would lead to a similar reaction toward all animals, *i. e.*, we would say: "All animals are men." Man may be said to be the richer concept, in that only a part of the reaction which determines an object to be a man is required to designate it as an animal. On the other hand, if we start with animal, then (except in case of the animals which are men) there is lacking the subject-matter which would permit the fuller concept to be applied. By supplying the conditions under which animal = man we get a reversible habit. The equation of technical science has just this character. It represents the maximum freeing or abstraction of a predicate *qua* predicate, and thereby multiplies the possible applications of it to subjects of future judgments, and lessens the amount of shearing away of irrelevancies and of re-adaptation necessary when so used in any particular case.

Formation and test of the hypothesis.—The formation of the hypothesis is commonly regarded as essentially different from the process of testing, which it subsequently undergoes. We are said to observe facts, invent hypotheses, and *then* test them. The hypothesis is not required for our preliminary observations; and some writers, regarding the hypothesis as a formulation which requires a difficult and elaborate test, decline to admit as hypotheses those more simple suppositions, which are readily confirmed or rejected. A very good illustration of this point of view is met with in Wundt's discussion of the hypothesis, by an examination of which we hope to show that such distinctions are rather artificial than real.

The subject-matter of science, says Wundt,[1] is constituted by that which is actually given and that which is actually to

[1] *Op. cit.*, Vol. I, p. 452 ff.

be expected. The whole content is not limited to this, however, for these facts must be supplemented by certain presuppositions, which are not given in a factual sense. Such presuppositions are called hypotheses and are justified by our fundamental demand for unity. However valuable the hypothesis may be when rightly used, there is constant danger of illegitimately extending it by additions that spring from mere inclinations of fancy. Furthermore, the hypothesis in this proper scientific sense must be carefully distinguished from the various inaccurate uses, which are prevalent. For instance, hypotheses must not be confused with expectations of fact. As cases in point Wundt mentions Galileo's suppositions that small vibrations of the pendulum are isochronous, and that the space traversed by a falling body is proportional to the square of the time it has been falling. It is true that such anticipations play an important part in science, but so long as they relate to the facts themselves or to their connections, and can be confirmed or rejected any moment through observation, they should not be classed with those added presuppositions which are used to co-ordinate facts. Hence not all suppositions are hypotheses. On the other hand, not every hypothesis can be actually experienced. For example, one employs in physics the hypothesis of electric fluid, but does not expect actually to meet with it. In many cases, however, the hypothesis becomes proved as an experienced fact. Such was the course of the Copernican theory, which was at first only a hypothesis, but was transformed into fact through the evidence afforded by subsequent astronomical observation.

Wundt defines a theory as a hypothesis taken together with the facts for whose elucidation it was invented. In thus establishing a connection between the facts which the hypothesis merely suggested, the theory furnishes at the

same time partly the foundation (*Begründung*) and partly the confirmation (*Bestätigung*) of the hypothesis.[1] These aspects, Wundt insists, must be sharply distinguished. Every hypothesis must have its *Begründung*, but there can be *Bestätigung* only in so far as the hypothesis contains elements which are accessible to actual processes of verification. In most cases verification is attainable in only certain elements of the hypothesis. For example, Newton was obliged to limit himself to one instance in the verification of his theory of gravitation, viz., the movements of the moon. The other heavenly bodies afforded nothing better than a foundation in that the supposition that gravity decreases as the square of the distance increases enabled him to deduce the movements of the planets. The main object of his theory, however, lay in the deduction of these movements and not in the proof of universal gravity. With the Darwinian theory, on the contrary, the main interest is in seeking its verification through examination of actual cases of development. Thus, while the Newtonian and the greater part of the other physical theories lead to a deduction of the facts from the hypotheses, which can be verified only in individual instances, the Darwinian theory is concerned in evolving as far as possible the hypothesis out of the facts.

Let us look more closely at Wundt's position. We will ask, first, whether the distinction between hypotheses and expectations is as pronounced as he maintains; and, second, whether the relation between *Begründung* and *Bestätigung* may not be closer than Wundt would have us believe.

As examples of the hypothesis Wundt mentions the Copernican hypothesis, Newton's hypothesis of gravitation, and the predictions of the astronomers which led to the discovery of Neptune. As examples of mere expectations we

[1] *Op. cit.*, Vol. I, pp. 454-461.

are referred to Galileo's experiments with falling bodies and pendulums. In case of Newton's hypothesis there was the assumption of a general law, which was verified after much labor and delay. The heliocentric hypothesis of Copernicus, which was invented for the purpose of bringing system and unity into the movements of the planets, has also been fairly well substantiated. In the discovery of Neptune we have, apparently, not the proof of a general law or the discovery of further peculiarities of previously known data, but rather the discovery of a new object or agent by means of its observed effects. In each of these instances we admit that the hypothesis was not readily suggested or easily and directly tested.

If we turn to Galileo's pendulum and falling bodies, it is clear first of all that he did not have in mind the discovery of some object, as was the case in the discovery of Neptune. Did he, then, either contribute to the proof of a general law or discover further characteristics of things already known in a more general way? Wundt tells us that Galileo only determined a little more exactly what he already knew, and that he did this with but little labor or delay.

What, then, is the real difference between hypothesis and expectation? If we compare Galileo's determination of the law of falling bodies with Newton's test of his hypothesis of gravitation, we see that both expectation and hypothesis were founded on observation and took the form of mathematical formulæ. Each tended to confirm the general law expressed in its formula, though there was, of course, much difference in the time and labor required. If we compare the Copernican hypothesis with Galileo's supposition concerning the pendulum, we find again that they agree in regard to general purpose and method, and differ in the difficulty of verification. If the experiment with the pendulum only substituted exactness for inexactness, did the

Copernican theory do anything different in *kind?* It is true that the more exact statement of the swing of the pendulum was expressed in quantitative form, but quantitative statement is no criterion of either the presence or the absence of the hypothesis.

Again, we may compare the pendulum with Kepler's laws. What was Kepler's hypothesis, that the square of the periodic times of the several planets are proportional to the cubes of their mean distances from the sun, except a more exact formulation of facts which were already known in a more general way? Wundt's position seems to be this: whenever a supposition or suggestion can be tested readily, it should not be classed as a hypothesis. This would make the distinction one of degree rather than kind, and it does not appear how much labor we must expend, or how long our supposition must evade our efforts to test it, before it can win the title of hypothesis.

In the second place, we have seen that Wundt draws a sharp line between *Begründung* and *Bestätigung*. It is doubtless true that every hypothesis requires a certain justification, for unless other facts can be found which agree with deductions made in accordance with it, its only support would be the data from which it is drawn. Such support as this would be obtained through a process too clearly circular to be seriously entertained. The distinction which Wundt draws between *Begründung* and *Bestätigung* is evidently due to the presence of the experimental element in the latter. For descriptive purposes this distinction is useful, but is misleading if it is understood to mean that there is mere experience in one case and mere inference in the other. The difference is rather due to the relative parts played by inference and by accepted experience in each. In *Begründung* the inferential feature is the more prominent, while in *Bestätigung* the main emphasis is on the experiential aspect.

It must not be supposed, however, that either of these aspects can be wholly absent. It is difficult to understand how any hypothesis can be entertained at all unless it meets in some measure the demand with reference to which it was invented, viz., a unification of conflicts in experience. And, *in so far*, it is confirmed. The motive which casts doubt upon its adequacy is the same that leads to its re-forming as a hypothesis, as a mental concept.

The difficulties in Wundt's position are thus due to a failure to take account of the reconstructive nature of the judgment. The predicate, supposition, or hypothesis, whatever we may choose to call it, is formed because of the check of a former habit. The judgment is an ideal application of a new habit, and its test is the attempt to act in accordance with this ideal reconstruction. It must not be thought, however, that our supposition is first fully developed and then tried and accepted or rejected without modification. On the contrary, its growth is the result of successive minor tests and corresponding minor modifications in its form. Formation and test are merely convenient distinctions in a larger process in which forming, testing, and *re*-forming go on together. The activity of experimental verification is not only a testing, a confirming or weakening of the validity of a hypothesis, but it is equally well an evolution of the *meaning* of the hypothesis through bringing it into closer relations with specific data not previously included in defining its import. *Per contra*, a purely reflective and deductive consideration which develops the idea as hypothesis, *in so far* as it introduces the determinateness of previously accepted facts within the scope, comprehension, or intension of the idea, is in so far forth, a verification.

If the view which we have maintained is correct, the hypothesis is not to be limited to those elaborate formulations of the scientist which he seeks to confirm by crucial tests. The

hypothesis of the investigator differs from the comparatively rough conjecture of the plain man only in its greater precision. Indeed, as we have attempted to show, the hypothesis is not a method which we may employ or not as we choose; on the contrary, as predicate of the judgment it is present in a more or less explicit form if we judge at all. Whether the time and labor required for its confirmation or rejection is a matter of a lifetime or a moment, its nature remains the same. Its function is identical with that of the predicate. In short, the hypothesis *is* the predicate so brought to consciousness and defined that those features which are not noticed in the ordinary judgment are brought into prominence. We then recognize the hypothesis to be what in fact the predicate always is, viz., a method of organization and control.